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Comment

Interactive comment on “A multiresolution spatial parameterization for the estimation of fossil-fuel carbon dioxide emissions via atmospheric inversions” by J. Ray et al.

Anonymous Referee #3

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Summary: The manuscript is well written and describes the application of using sparse wavelets based on inventories and proxies to represent fossil fuel emissions in an atmospheric inversion framework. After addressing the minor items listed below, the paper could become a valuable addition to the literature on the inversion of anthropogenic sources.

1. The authors obtain spatial sparsity fractions of about 80% for representing Vulcan emissions using wavelets. While these compression rates may, at first, seem phenomenal, they are not surprising given that most of the gridcells in the region of interest in Fig. (1b) do not contain significant levels of fossil fuel CO₂

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emissions. One alternative and naïve approach to wavelets would be to consider only those gridcells, or some aggregated set of gridcells (e.g. 4×4), that contain emissions above a specified level (e.g. $>1\%$ of the max). Another approach could be to prescribe spatial basis functions that have areas proportional to population (i.e. small areas for large metropolitan regions, and large areas for rural regions). I surmise that these naïve approaches would also lead to large sparsity fractions or reductions in dimension. To better illustrate the strengths of their wavelet approach, I recommend that the authors devise a naïve metric of sparsity and compare and contrast their numbers to this metric.

2. On page 1300, lines 26-28, the authors note that the deterministic nature of their presented method is a drawback. Without quantified confidence intervals and uncertainties, it is difficult to ascertain the significance of the inversion results (e.g. as shown in Fig. 7). The authors should run additional inversion tests that vary ϵ_2 , ϵ_3 , and other relevant parameters, and then report on the sensitivity of their results to these variations. Furthermore, the manuscript should contain a discussion of the errors described in items 3 and 4 below.
3. Underreporting is a known and persistent bias in using inventory-based estimates for monitoring anthropogenic emissions. The authors should describe what happens to this important source of error when using nightlights and BUA as spatial proxies for inventories in their wavelet representation. Does this error become confounded with separate errors in the proxies and can it be attributed to the inventory post-inversion? In a similar vein, are there errors in the proxies (e.g. clouds obscuring nightlights) that become confounded with the inventory in the wavelet representation?
4. The inversions are performed assuming a perfect atmospheric model. In reality, atmospheric models contain biases and other imperfections that can severely limit the ability to invert for regional scale surface emissions. The authors should

- describe how model imperfections could be included the inversion (e.g. as an extra term in Eq. 5) and how they might be confounded with other errors in their sparse wavelet representation.
5. The inversion results for the U.S. shown in Fig. (7a) exhibit pronounced seasonality, with small error reductions during periods 7 and 27, and large error reductions offset by 2-3 months during periods 15 and 35. The time dependence of the inversion suggests the presence of multiple time scales of interest that do not seem to be represented in the inversion demonstration. Although the wavelet coefficients in Eq. 7 vary with time (i.e. they contain index k), the wavelets themselves do not (i.e. do not contain index k). Are the spatial distributions of the nightlight and BUA proxies fixed for the year? If so, would introducing time-varying spatial distributions of these proxies reduce this seasonality? Please respond and include appropriate discussion in the manuscript.
 6. In a comment related to item 5, fossil fuel emissions also vary over multiple time scales (daily, weekly, monthly, and yearly). Although the manuscript adequately describes the various spatial scales (and "spatial" is specified in the title), the discussion of multiple time scales is haphazard. I recommend including this discussion in the manuscript and describing how the sparse wavelet technique can (or cannot) be extended to capture multiple time scales. Making a clearer distinction between multiple time and space scales will also be helpful.
 7. The manuscript attributes inversion differences to differences between EDGAR and Vulcan emissions. The authors should also compute and report the raw differences between these two emissions inventories before they are used in the inversion demo.
 8. The synthetic observations used in the inversion, which are first introduced on page 1291 and later discussed on page 1295, should be described more clearly and in more detail. Were the elements of the sensitivity matrix \mathbf{H} generated for

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another problem and adapted for this manuscript or were they computed specifically for this paper? As a numerical verification test, do the sensitivities multiplied by the Vulcan fluxes equal the concentrations obtained from a single forward simulation using Vulcan (i.e. does y equal Hf as given in Eq. 5)? More information about the WRF setup would also be useful (What lateral boundary conditions were used to generate the winds? What physics packages options were used? and so on).

9. The authors analyze and display (Fig. 3) the statistics of non-zero wavelet coefficients. To help with visualization, it may also be useful to display maps of a few of the major features obtained from the wavelet decomposition.
10. On page 1288, line 13, the authors incorrectly associate static sources with emissions from highways. While it is true that highways are fixed, the traffic flow along them is not. CO₂ emissions from traffic is usually categorized as mobile and non-stationary.
11. Some of the figure and captions could or should be modified for clarification and easier comparison. Can you display CASA emissions in Fig. 1a for the same time period as Vulcan emissions? Please make Fig. 5 larger. The figure labels in Fig. 6 state that the emissions are for a single 8-day period, while the caption mentions emissions for one year and a single period (remove the “over one year”). The label in Fig. 9 shows period 34, while the caption states period 31 (fix the typo or make consistent).
12. Please add “et al” to the Friedlingstein reference on pages 1278 and 1303. Also, according to recent work (see Fig. 1a in Regnier et al, doi:10.1038/ngeo1830), fossil fuel emissions are not the largest net carbon flux at the atmosphere-surface interface. Please revise the second sentence in the Introduction accordingly.

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